increasing year by year, and their catch, especially in the early spring, being always very great.

. . . .

"VI. The decrease of the fish may be considered as due to the combined action of the fish-pounds or weirs, and the blue-fish, the former destroying a very large percentage of the spawning fish before they have deposited their eggs, and the latter devouring immense numbers of young fish after they have passed the ordinary perils of immaturity."

As Prof. Baird goes on to remark that there are no measures at command for destroying the blue-fish, even if that were desirable, and as the blue-fish was once far more abundant than it is at present, while other fishes were also more numerous, I cannot see that I made any mistake in stating that "over-fishing" was unquestionably assigned as "the chief cause" of the decrease in American sea-fisheries.

Lastly, Mr. Holdsworth says that the question lies between the late Royal Commissioners and myself. It was under this belief, holding him as their secretary to be their mouthpiece, that I took some trouble to reply to his first letter. Had any one not in that position challenged my remarks I should, perhaps, have not felt myself bound to give my reasons for the faith that is in me. He asserts that I have "no practical acquaintance with the subject." Possibly he considers that qualification limited to those who have been named in a Fishery Commission. In such case I certainly have none. He further charges me with using the Index to the Evidence as my "sole guide." Here I must venture to contradict him. I have used that Index, indeed, but much as Norwegian fishermen use the "water-telescope"—to look into the teeming depths of evidence below, unobstructed by the surface ripple of a Report.

To sum up. Your readers are aware that I originally treated of the Fisheries question as part of a much wider subject on which I felt constrained to speak my mind at a fitting opportunity. I have yet to learn that the Report of a Royal Commission is beyond the reach of fair and cool criticism, or that it is obligatory on all men to accept that Report as a revelation from supreme intelligence. My criticism of this Report was, I venture to think, not unfair, and it was not made in hasty warmth. Some ten years had passed since I adopted the opinions I hold, and the time had come when, as I thought, I could not help uttering them, nor does it seem to me that an unfitting occasion was offered by a meeting of the British Association. The decision of the question whether there is and has been "over-fishing" or not is hardly helped by the reiteration of the passage with which my friend ends his rejoinder.

Magdalene College, Cambridge, ALFRED NEWTON
December 15, 1876

Ocean Currents

AGREEING in the main with Mr. Digby Murray's argument on the subject of ocean currents in NATURE (vol. xv. p. 76), I am the more disposed to criticise some of the statements with which it concludes, as put forward too strongly, to say the least.

I would ask for the "absolute proof" which Mr. Digby Murray supposes to exist, that (1) the upper-current return-trades "flowing from the equator descend again to the surface of the ocean on the polar sides of the calms of Cancer and Capricorn," and (2) "that these equatorial currents, subsequent to their descent on the polar sides of the calms of Cancer and Capricorn, are known as the westerly winds of the temperate zones." That these statements represent the prevailing opinion on the subject I readily admit, but I have ever looked in vain for any convincing arguments in their favour.

As regards the hypothesis that the trades cross one another in the region of equatorial calms, I may perhaps be permitted to quote some remarks of my own, made two years ago (*Symons' Met. Mag.*, vol. x. p. 37), since subsequent study has tended to confirm the doubts which I then expressed :—

"Maury's hypothesis, that the surface trade-wind of one hemisphere becomes the upper-current return-trade of the other ('Physical Geography of the Sea,' sec. 122 to 139) was in all probability originally suggested by the well-known fact that over the southern portion of the N.E. trade a S.E. upper-current prevails, and over the northern portion of the S.E. trade a N.E. upper-current, though he lays most stress on the arguments which he draws from the greater rainfall of the northern hemisphere (sec. 169 to 186), and from Ehrenberg's examination of the African air-dust (sec. 266 to 296).

"A seaman on approaching the doldrums, commonly notices a current overhead blowing at an angle of about 90° with the surface-trade; he is aware that this upper-current coincides in direction with the trade on the other side of the doldrums, and that in the calm belt itself, there is an upward motion of the atmosphere. It is, therefore, not unnatural that he should conclude that the upper-current which he observes is a poleward extension of the opposite trade in the higher regions of the atmosphere. It may also, I think, be admitted that the rapid and suddenly shifting cloud-currents, often observed over the region of the doldrums, are somewhat in keeping with Maury's idea of 'curdles,' or alternate strips of air.

"I would suggest that this hypothesis (which many subsequent writers have been surprisingly ready to adopt) may, perhaps, be subjected to a crucial test, if an answer can be given to the following query :—When the south-east trade draws so far to the north as to be deflected into a south-west surface wind, what is the prevalent direction of the upper-current over the southern portion of the north-east trade? If it runs from south-west it will be difficult to resist the conclusion that Maury is right; if from south-east it will appear probable that the upper-current is (principally at least) the north-east trade, deflected in the first part of its return course towards the north-west, just as it is in the subsequent part towards the north-east.

"Perhaps some meteorologist can give a definite answer to this question. The published data for its solution appear rather scanty; but, so far as my own limited information goes, the observations are generally rather adverse to Maury's theory."

I would now ask what proof exists that the upper currents from the polar depressions and those from the equatorial depression cross one another in the calms of Cancer and Capricorn so as subsequently to become the trades and anti-trades respectively? Since these upper-currents are understood to meet at the belts of tropical calms and there to descend, it is surely "more reasonable to suppose that their currents intermingle and that their mixed volume is then drawn off north and south, as required to restore the equilibrium of the atmosphere." These are Mr. Digby Murray's words in reference to the equatorial calms, and I fail to see why they will not apply to the calms of Cancer and Capricorn.

The whole question of the cause of the prevailing south-west and north-west winds of the north and south temperate zones, and the relation which these bear to the polar areas of barometric depression, may be regarded as fairly solved by the researches of Mr. Ferrel, Prof. J. Thomson, and others. As regards the great intensity of the Antarctic, as compared with the Arctic depression, and the superior force of the westerlies on its border, there is surely *primâ facie* ground for believing that these are mainly due to superior evaporation in the water-hemisphere generally. (I say "mainly," because it seems probable that the comparative absence of surface-friction experienced by the atmospheric currents in that hemisphere tends to intensify the Antarctic depression.) That the evaporation from the warm surface-water of the North Atlantic is in excess of that from the relatively cold surface-water of the South Atlantic, may be readily admitted; but the Atlantic represents, after all, only a small portion of the surface of the globe. Will anyone maintain that the evaporation from the whole continent of Asia is equal to that which takes place from the corresponding area of the South Indian Ocean? W. CLEMENT LEY

Solar Physics at the Present Time

HAVING now read the *Astronomical Register's* more extended account of the November meeting of the Royal Astronomical Society, I found it very confirmatory of NATURE's shorter, but more quickly produced, summary of November 23, especially in what was said in the discussion upon Prof. Langley's (United States) paper on Sun-spots and Terrestrial Climate. Will you kindly allow me to remark :—

1. I am extremely glad that Sir G. B. Airy is now finding from the deep-soil thermometer observations at Greenwich that, whatever may be the interior temperature of the earth, and the terrific manifestations of it in some special volcanic localities abroad, yet all the remarkable changes and occasional abnormal elevations of temperature in the Greenwich soil *come from without;* for, Sir, that is precisely one of the earliest conclusions which I deduced for the Edinburgh soil, from the longer series of similar deep-soil thermometers there, and which I had the honour

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of communicating to the Royal Society of London *six* years ago.

2. I am also very glad that Prof. Langley has proved by his most refined and crucial observations on the direct radiating powers of spot, penumbra and photosphere surface, combined with the continuous register of the amount each day of the spotted portion of the sun, that sun-spots have no sensible power in themselves for producing any notable change on terrestrial climate; because, Sir, in that paper of six years ago, and still in the hands of the Royal Society, I deduced that sun-spots were consequences, rather than causes, of the great periodical waves of heat which come upon the earth from without; and I proved that conclusion three times over, or for three successive cycles of the eleven-year sun-spot period.

3. As we are now on the commencement of another of those cycles, I must regret that the chief speakers seemed to intimate that almost all their idea of further investigation into the origin of those mysterious heat-waves received by the earth from without and apparently from the sun, centred in causing to be made *more sun-spot observations;* for, Sir, not only did it sound very much like proposing to lock the stable-door as soon as it should be announced that the horse is no longer therein, but there are further features accompanying those occasional great heat-waves, showing that they must originate in something much more intense, violent, and complicated than those comparatively harmless little phenomena, the dark spots. PIAZZI SMYTH,

Edinburgh, December 5 Astronomer Royal for Scotland

Radiant Points of Shooting Stars

BETWEEN October 13 and November 28, watching for forty-nine hours, I observed 367 shooting stars, 306 of which were well seen and their paths registered. On going carefully over them some thirty-five radiant points are shown, about twenty-five of which are good positions, while the remainder are open to more or less doubt. The following are the twelve principal ones:—

Dates.	Position of Radiant Point.		No. of Meteors.	Notes.
	in	at		
		R.A. Decl.		
1. Oct. 21–Nov. 20 ...	Taurus	60° + 19°	25	Many fine meteors, mean of three showers.
2. Sept. 17–Nov. 8 ...	Pisces	15 + 11	17	Mean of two showers, very slow meteors.
3. Sept. 20–Oct. 25 ...	Musca	46 + 26	19	Small rapid meteors.
4. Oct. 13–Nov. 28 ...	Lynx	125 + 47	17	White rapid meteors, mean of two showers.
5. Sept. 18–Nov. 8 ...	Cassiopeia ...	15 + 52	20	Small meteors, mean of two showers.
6. Oct. 25–Nov. 8 ...	Gemini	110 + 22	11	Mean of two showers, very swift meteors.
7. Nov. 20–28	Leo Minor ...	155 + 36	21	A fine A. M. shower.
8. Nov. 20–28	Ursa Major ...	208 + 43	12	Just before sunrise, small, short meteors.
9. Nov. 7–8	Camelopardus...	69 + 66	10	An evening shower.
10. Sept. 17–Nov. 20...	Lacerta	345 + 43	10	White rapid meteors, mean of two showers.
11. Nov. 22–28	Auriga	78 + 43	9	White rapid meteors.
12. Oct. 17–25	Orion...	88 + 17	8	A well-known Oct. shower.

Several of these positions are the mean of two showers, one seen in October the other in November, and evidently identical. No. 7 in the list is possibly new, and was a very active shower from near Leo; the meteors were rapid and white, leaving phosphorescent streaks. No. 8 is quite new, and three other radiants found on the mornings between November 19 and 28 are new. They are:—

	Position.	R.A.	Decl.	
1.	Near τ Leonis ...	170	+ 4 ...	6 meteors, very swift.
2.	In Sextans	153	S 1 ...	6 meteors.
3.	α Boötis	212	+ 18 ...	7 meteors.

The positions given are in most cases very accurate, and each of them represents a well-marked shower. The new radiants are visible preceding sunrise, and this may account for their having previously escaped detection.

Generally the meteors of October–November were very small. The magnitudes of the 306 registered were:—

× or = 1st mag.	2nd mag.	3rd mag.	4th mag.	5th mag.	6th mag.	Total.
19 ...	49 ...	61 ...	107 ...	65 ...	5 =	306.

I have lately found meteors very much more frequent after midnight than before it. In November, 13¾ hours watching, P.M., gave 79, while 12 hours watching, A.M., gave 133. Thus I have noted about double the number in the mornings than in the evenings. I found a similar difference in October, though have made no special comparisons to find if it has also been shown in the other months of the year. I usually find meteors show a progressive increase in numbers as the night advances, being at a minimum early in the evening hours and at a maximum just before the morning twilight.

Ashleydown, Bristol WILLIAM F. DENNING

The Atlantic Ridge and Distribution of Fossil Plants

IT has occurred to me that the discovery of the narrow belt of suboceanic highlands extending in a sinuous course down the length of the Atlantic, as shown in the *Challenger* chart, removes a difficulty that has been present to students of fossil botany. When the area was land these hills would probably form a ridge sufficiently high to have a temperature cool enough to explain the migration across the tropics of plants living in a temperate or even cooler climate. M.

Antedon Rosaceus (Comatula Rosacea)

THE letters of your correspondents with reference to the above, seem to me to fail to prove that there is any public recorded instance of its capture in the stalked (brachial) form at Torquay before the instance noted by the Birmingham Natural History and Microscopical Society in 1873. Of course if Prof. Allman took a specimen in the pre-brachial stage there in 1863, and Mr. Gosse the adult animal in 1864, the stalked form (brachial) must have been there as well, but was probably overlooked.

As to the change of name to which Mr. Thomas R. R. Stebbing objects, I certainly think that Dr. Carpenter, in his monograph before referred to, has deduced ample reasons for the substitution of *Antedon* for *Comatula*, "on the grounds of priority, in accordance with the rules of zoological nomenclature, and in concurrence with the views of Dr. J. E. Gray, Sir Wyville Thomson, and the Rev. A. Merle Norman."

Birmingham, December 10 W. R. HUGHES

"Towering" of Birds

MY experience goes to show that the towering action, although most frequent in the gallinaceous birds, is by no means confined to them. In the first case which came under my notice the bird was the common godwit. It was feeding on the border of a marsh, and I being very young at the time, committed the unsportsmanlike act of firing at it on the ground. Immediately on being hit the bird rose perpendicularly to a height of about 30 feet, then turned over on its back and fell dead almost on the spot from which it started.

Since then I have seen the same movement in the dunlin and some other species of *Tringa*, in the sanderling, the whimbrel, and, if I recollect rightly, in the lapwing plover, but in no other birds, excepting of course those mentioned by Mr. Romanes.

I have never seen a towering shore bird, after being struck, fly any considerable distance before towering, and those which have towered directly on being hit have always received a slight wound at the base of the brain, but there is little doubt that pulmonary hæmorrhage is the principal cause of this curious action.

December 12 F. W. MILLETT

THE SPECTRUM OF THE NEW STAR[1]

NOTWITHSTANDING the bad weather and the feeble light (4th to 5th mag.), I have been able to investigate pretty completely the spectrum of the light

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[1] Sur le spectre de l'etoile nouvelle de la constellation du Cygne. Note by M. A. Cornu, read at the Paris Academy of Sciences, December 11.

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